The Sidereal Messenger.

CONDUCTED BY WM. W. PAYNE,

Director of Carleton College Observatory.

JULY, 1886.

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By faith we understand that the worlds have been framed by the word of God, so that which is seen hath not been made out of things which do appear.

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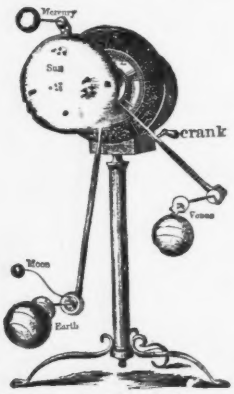

HELIOTELLUS.

When the Earth's axis is pointed to the north, it will continue so pointing throughout the revolution, and will be in the Ecliptic. The Earth rolls over from west to east, and if the Equator be continued to the sky, it will meet the Equinoctial. So with the Ecliptic, if continued, it will come near to the Moon, near to Mercury, near to Venus, and always to the Sun, for the ecliptic plane.

I have now in my possession all the Heliotelluses for sale, made with a set of tools costing $25,000, which tools were afterwards destroyed by fire. They were so accurately made that the Heliotellus cannot now be duplicated for less than $250 each. The greatest impediment I find in their introduction is the Tellurian, which makes a false showing of the heavenly movements. It is a device in which the Earth's axis wabbles around the zenith and never points to the north. This is the greatest bearier to the comprehension of this most sublime of the sciences. The Heliotellus shows so near the truth that it is not hard to comprehend.

In high schools, seminaries, colleges, and all places of learning we find many globes and maps of the earth, but where can one be found having the Equator of the Earth so constructed that if continued it will meet the Equinoctial in the right place on the sky? Every child should have a truthful understanding of science. The Ecliptic should be correctly undertood. All instruments which show imperfect teaching are hurtful; those which teach correctly are useful. The one should be rejected, the other sought for, and when found should be prized even as a "pearl of great price." Three hundred such I now possess, all perfectly made, and I now propose to sell two hundred at the reduced price of $20 each, or for $65, the price of one, I will send four, each well packed in a strong box to carry by express anywhere. Address,

HENRY WHITALL,

BELVIDERE SEMINARY, BELVIDERE, N. J.

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The Sidereal Messenger,

CONDUCTED BY WM. W. PAYNE,

Director of Carleton College Observatory, Northfield, Minnesota.

"In the present small treatise I set forth some matters of interest to all observers of natural phenomena to look at and consider."—GALILEO *Sidereus Nuncius*, 1610.

VOL. 5, NO. 7. JULY, 1886. WHOLE NO. 47.

PULKOWA DOUBLE-STAR WORK.

[By the kindness of a friend, we have read a private letter written by OTTO STRUVE, some years ago to S. W. BURNHAM, of Chicago, concerning double-star studies. With pleasure and MR. BURNHAM'S reluctant consent, after making at first a positive refusal, we give selections from the pen of Pulkowa's scholarly astronomer.—ED.]

First of all, accept my most sincere thanks for your letter, and the assurance that it will afford me great pleasure to correspond with you upon the subject of Double-Stars,—a subject which has constantly occupied my attention, with short interruptions, since my youth, now more than forty years, and most intimately accompanied the growth of my scientific life. To have fellow laborers in this field, greatly enhances my pleasure, especially when they take hold of the subject as earnestly and eagerly as you and Baron DEMBOWSKI. Our efforts, just because they are not identical in their directions, complement each other happily, and then there can be no mistake that in such ways, by united strength, happy results are brought to light.

From the various writings published by me, you will have recognized in general the character of my efforts in this department—the investigation of the motions of the northern hemishere in single systems. Still more evident will this appear, when the whole series of observations which I began to print at the end of last year, shall lie before you, published in full. But in order to render possible, even now, a deeper insight into this work, and, as the printing progresses, to gratify

the wish expressed in your letter for the communication of new measures, allow me now to send you the accompanying twenty proof sheets of that work. I will, if agreeable to you, continue sending them at proper intervals. To explain: I have divided the publication into four sections, the first of which embraces the W. STRUVE stars; the second the O. STRUVE stars; the third the double-stars, which have been discovered by other astronomers (J. HERSCHEL, BURNHAM, DAVIES, ALVAN CLARK); the fourth, doubles of the fifth and sixth classes of HERSCHEL, and other stars with great proper motion. Finally, during the preparation for the press, I shall supply the most recent measures as a supplement to all four of the preceding sections. The publication is divided into two volumes (Volumes IX and X of the Pulkowa Observations) and, as an introduction to the first part, will contain in addition to the historical portion, the investigations concerning personal and instrumental errors, while the introduction of the second part will contain the comparison of the relative motions with the absolute ones, which may have been especially determined for the purpose with the meridian instrument here. Unfortunately, the work has suffered many interruptions, and they still threaten it even now, since I am able to devote to it only a comparatively small portion of my time, on account of other duties—director for twenty years of the Observatory, and for thirty-five years Scientific Conductor of the extended geodetic and geographical labors in the Russian realm.

I cannot but acknowledge the validity of the grounds which have induced you to include in your forthcoming General Catalogue each and every object which has been designated as a double-star by any astronomer, those cases naturally excepted where the observer himself has afterwards pointed out an object which he had introduced erroneously. Through this completeness, and still more through the care which you have exercised in the identification of the objects hitherto uncertain, and in the fixing of the positions, your work will undoubtedly have a very high scientific merit; and it gives me pleasure to

extend to you, in advance, thanks for it. According to my idea, completeness in reference to what is designated by any one anywhere as a double-star, is of less importance than a knowledge as complete as possible of what the sky presents only within limits rigidly fixed in reference to distance and brightness. Perhaps you might even yet have the desire to elaborate your work in the given direction, and therefore I will take the liberty of laying before you my views in reference to it. As a starting point we lay down the proposition : double-stars are such stellar pairs as move about one another in orbits under the special condition of mutual attraction of the visible components. Thus we possess already in the researches of my father (Cat. Novus, Mens. Mic, and especially Pos. Med.) frequent evidence to confirm our judgment in regard to the limits, which, according to the laws of probability, and corresponding to our present proofs of gravity, should be adopted for a double-star catalogue, in this more limited sense.

Such a double-star catalogue is a real need in order to give relative limits to those who wish to engage in micrometrical measurements, and to diminish the waste of energy. It would furnish directly a more secure foundation, now almost entirely wanting, for rendering computations according to the theory of probabilities valuable, and leading to wider and positive results. In this case, however, care should be taken that the catalogue be so constructed that it should receive no modification for a long interval of time. A simple division of your present work into two parts, say into special double-stars, and such stellar pairs within four minutes distance, as have been noted and measured by different astronomers — or say in *Duplicæ*, *Lucidæ and Reliquæ*, would not in itself alone lead to the limits; since hitherto, definite limits in reference to brightness and distance have been accepted scarcely by any one observer. Thus first of all, a systematic revision of the heavens would have to be carried through, in order to obtain a uniform material. An important auxiliary for this, with reference to the brightness of the objects to be investigated, has now been furnished through the Bonn *Durchmusterung*. On

the basis of your own work, you will probably find the statement true, that to magnitude 7.0 (according to W. STRUVE) and distance 16 seconds in the northern hemisphere, only a small percentage are still unnoted. Within these limits further accsessions through a new revision, could only be very few in number; although as the systems discovered by you (*e. g. β Delphini*) show that, even among the few objects hitherto unnoted, perhaps especially interesting systems might be discovered.

But I think it would be desirable to extend the limits, and perhaps to accept for the principal stars 8.0 (according to ARGELANDER) as the limit of magnitude; for the companion, without regard to magnitude, 32 seconds as the limit of distance, by which named distance the ratio of the physical to the optical double-stars, as experience teaches (*Poss. Med.* page CCXXXX) is at least in the case of the brighter companions very nearly 1 to 1. But beyond 32 seconds the ratio rapidly diminishes, while within that limit the number of optical doubles is proportional to the square of the distance, and soon it would increase to a figure well nigh immeasurable. Beyond these limits, it would be important to establish limits in respect to the magnitude of the companions. Exception should be made of these doubles for which, as *e. g. Regulus*, *40 Eridani*, etc., the physical connection, in spite of the faintest of the companions, is already assured, or at least very probable. For discovering such exceptional cases, which are indeed of great interest, such a Catalogue as the one you are now finishing would be certainly of great use. But the number of these exceptional cases is infinitesimally small, compared with the mass of those objects which could be registered, being only optically near one another.

My opinion, to sum up, is then as follows: a double-star cataloge in the specified restricted sense is a real desideratum, but it should be based upon a systematic revision. What limits should be retained for this naturally stands open for further discussion. In the first place, it would be necessary to find the forces for such a work, which, even within the limits of 8.0

magnitude for the primary, and reaching from the North pole to 30 degrees south declination would require the setting the telescope upon, and careful examination of about 80,000 objects, and which therefore ought not to be laid upon a single observer. But if three or four observers would be willing to share in the work, then it could be far advanced in a couple of years. I have already thought of directing the refractor here upon this work, as soon as I finish my present publication, which perhaps will still require two years, and demand various supplementary investigations. But here we certainly would be limited entirely to the northern hemisphere. Now perhaps you yourself would like to take part in such mutual work, and I should think that one or two especially interested in this subject might be found in Germany or England. Be so kind as to give me your opinions upon these suggestions now thrown out.

The task which you have set yourself at in the completion of your General Catalogue, namely, of collecting as fully as possible, and with all concordance, whatever is designated anywhere as a double-star, and to clear up by personal observation all doubtful estimates therein, and to correct the chance errors which have occured, is truly no easy thing; and I wonder at the energy which has enabled you to advance through the work in a few years. Twenty-five years ago I undertook to investigate only those stars in J. HERSCHEL which were not already included under the W. STRUVE, or O. STRUVE stars, but which according to the given description, might be reckoned as far as possible among double-stars in the restricted sense, as also those casually noted as double by BESSEL and ARGELANDER in their zone-observations, in all six hundred and fifty celestial objects, and I remember that this work cost me much time and fatigue. And what an infinitesimally small fraction has that work of mine been, in comparision with that which you have accomplished in comparatively so short a time!

Besides this, you have undertaken the work of enriching your catalogue with a description as exact as possible of the pairs, and statement of their relative positions, as well as with reference to all measurements, whereby the labor of later

workers in orbits will be much lessened. In this part of the work, it will not be entirely agreeable to you to learn that my work, which will contain by rough estimate some 10,000 observations, generally made under good atmospheric conditions, will be only partially accessible to you before the printing of your catalogue.

In the fourth section you will also find a series of objects which are probably not mentioned in your catalogue, but possibly belong in it according to the plan of your work. In regard to these, however, I do not think they ought to be numbered with double-stars. I have for a long time connected by frequent careful micrometric measures about one hundred stars, distinguished by very great proper motions, with neighboring, and generally very faint stars, in order to investigate their connection for the purpose of determining their parallaxes. But to number such stellar pairs among double-stars would be to go far out of the way. Nor have I permitted myself for a similar reason to designate as new, distant companions of known doubles, although at the time I have connected them by measures.

The numbers O. STRUVE 540 to 549, in regard to which you desire more explicit information, I hastily collected only a short time ago, at the desire of Baron DEMBOWSKI, without being able to undertake in reference to them a careful comparison with other catalogues. I take the liberty of copying out of my record books the following details for your use. [These details omitted.]

As you have also consulted the *Bulletin* of the St. Petersburg Academy, no other publication occurs to me which has appeared here, that you have not yet referred to in your work. I will take the liberty however to add a copy of the catalogue *Novus*, and the *Positiones Mediæ* to our next package forwarded through the Smithsonian Institution. The Catalogue of 1843 is unfortunately entirely out of print. Perhaps I might also procure for you a copy of the volumes of the Dorpat Observations, which contain MADLER'S Double-Star Measures, if you do not yet possess them. MADLER'S measures have in-

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deed only a very subordinate value, still they belong to the history of this subject.

I thank you for sending your paper upon the so-called HERSCHEL'S General Catalogue. Although as you correctly remark, this publication is worse than useless, I could not omit its notice in the *Viertiljahrsschrift* on account of HERSCHEL'S name.

Recently, together with the further editing of my observations, I have been specially occupied with investigations on the orbit of 61 *Cygni*. During the last decade, both components have shown a decided diminution of their motion in a straight line. Under a supposition of a uniform motion in a circular orbit, this would give a period of revolution of abont six hundred and fifty years.

I must beg your pardon for writing in German, my mother tongue, although in earlier years I corresponded regularly in English with my English friends, and do this even yet with some of them, *e. g.*, my old and honored friend, Sir GEORGE AIRY. But I find that with an increasing age, correspondence in foreign languages becomes a continually increasing labor, especially, if, as with me, duties multiply, and do not permit proper time and care to be given to each letter. In reference to reading foreign languages, I find, however, it will make no difference if your answers follow in English.

Till to-day your letter has laid in the portfolio devoted to unanswered communications, and I rejoice that I can now take it out, since I hope that the present letter, which has already grown very long, will give you clear evidence of how highly I value your labors, and at least free myself from the reproach of indolence in writing to you. With the highest esteem, yours with true devotion, OTTO STRUVE.

Among the interesting papers in a late issue of *Nature* (June 3) are those on *Sunspots and Prices of Indian Food Grains* by Frederick Chambers, of Bombay, and the *Physical Appearances of Mars in 1886* by W. F. Denning, of Bristol, England. This double number is unusually good.

DIFFRACTION.

By HENRY M. PARKHURST.

After making many observations with different forms of photometers, founded upon the principle of the reduction of the aperture near the eye-piece, I at one time suspended them for several years, chiefly in consequence of my inability to explain the contraction of my scale as compared with that of observers using the principle of polarization. During the last three years I have been observing variable stars, sometimes by comparison and at other times photometrically, and in the latter observations have again met the same difficulty. This became especially glaring in the attempt, by means of a new logarithmic cap with a much narrower opening than I had before used, to connect the stars of the Harvard Photometry with my comparison stars. The brighter stars would disappear with an aperture 50 per cent greater than they should have done with the accepted ratio of 2.512.

The photometric results of Prof. Langley, in determining the reduction of light in its transmission through wire grating (*American Journal of Science and Arts*, Sept., 1885,) suggested the query whether diffraction was not the source of my difficulty, since the logarithmic curves for the brighter stars approach each other very closely, in the case of the last cap above referred to, within 1-30th of an inch. I was unable to learn that there was any mathematical way to ascertain the amount of loss of light by diffraction in a cap of given dimensions, and attacked the problem in the following way.

I reasoned that the light cut off in passing a straight edge, for instance, the side of a large, square cap, would equal a certain definite width, x, so that with such a cap the effective light would correspond with an aperture $2x$ smaller, without diffraction. In order to determine x, we must know approximately the law of the extinction. I assumed for trial three different laws, involving the first, second and third powers of the distance from the diffracting edge. The first power in my

formula indicated uniform extinction over the whole width x affected. The second power indicated an increase in approaching the edge proportionate to the distance from the limit of effect y; and the third power increasing in proportion to the square of the distance. Neither of these was a plausible theory, but I hoped that for distances greater than 1-30th of an inch one of them would give satisfactory results, and I could not devise any theories of equally easy computation which would answer that purpose.

Covering the open space at the end of the curve, to increase the effect and to allow of the observation of a much wider range of brightness, I made three series of observations, two with the above cap and the third with the next narrowest cap, and computed the apertures at which the different stars disappeared. Then assuming different values of x and y, and the three different powers, I satisfied the observations as well as I could. The result was that whichever power was used the value of x came out .04 inch as the least that would be admissible, and that a change of the power made no appreciable practical difference with the logarithmic caps that I employed. It varied largely, but with the second and third powers the proportion of the diffraction beyond the distance .08 inch was so small that the errors of observation made it doubtful, even with the narrowest cap, which need no longer be used. As the practical result, I have enlarged the openings of my logarithmic caps by adding .04 inch all around the margin. It remains to be seen whether in actual, long continued use it will be found necessary to increase the value of x, which is the only question necessary to be considered in ordinary photometric work.

Although, from the peculiar shape of my apertures, the question of diffraction is especially important in my apparatus, all other modes of determining magnitudes by measurement of aperture are subject to the same errors, especially when those measured apertures are small. Assuming 9.2 mag. for a circular aperture of one inch diameter, clear of diffraction, there should be an allowance on account of diffraction for the

different magnitudes as follows: 14 mag., —.02; 12 mag., —.05; 10 mag., —.12; 9 mag., —.18; 8 mag. —.28; 7 mag., —.44; 6 mag., —.65; 5 mag., —.93. From this it appears that the actual magnitudes corresponding to the last two apertures would be 4.07 and 5.35; an actual difference of 1.28 being contracted to a measured difference of 1.00.

It is manifest that this error is not confined to the brighter stars, when fainter stars are measured by means of small apertures; for if, by cat's eyes, an iris diaphragm, or any similar plan, the actual aperture used is less than an inch, the error of the scale becomes noticeable, no matter how faint the stars which are measured. For instance, by a comparison of the observations with Photometer I, in the third report of the Committee on Stellar Magnitudes, with the observations with the meridian photometer and with the wedge, it will be found that in the average the scale is contracted half a magnitude for each star of the 24 stars. Diffraction would produce a contraction of the scale; but it would require a reference to the original observations to ascertain the degree of correspondence in the amount with that indicated by my own observations. It will be noticed that the large discrepancies between the wedge and Photometer I, occur with the brightest stars observed; whereas it would seem that the brighter stars must have been observed with a larger aperture, making the diffraction the least. Probably this is to be explained by the fact that the observations were differential, and that the whole series with Photometer I was increased beyond what it should have been, by a constant amount; so that the subtraction of that amount would make the observations of the bright stars consistent, and throw the discrepancy upon the faint stars where the effect of diffraction would be greatest.

The Lick Trustees have decided to purchase from Messrs. Feil and Mantois a 36-inch crown disk, which was made by them at the same time with the crown disk of the objective now in the hands of the Clarks. The Clarks "have received the order to figure this disk as a third (photographic) lens for the large objective."

A SHORT METHOD FOR COMPUTING OCCULTATIONS.

CHAS. L. WOODSIDE.

FOR THE MESSENGER.

The more general observation of occultations of stars by the moon by observers in all parts of the earth is, it seems to me, very much to be desired; for occultations, accurately observed, furnish information to the investigator of the moon's motion of a character nowhere else to be obtained. Comparatively few occultations are observed, however, owing principally to the large amount of time and labor necessarily consumed in the computations, all of which is lost should the sky be obscured during the occultation. The method here presented will, in a great measure, I think, avoid this difficulty, for so little time (not more than fifteen minutes) is consumed in computing an occultation by this method that the computation need not be made until just before the occultation takes place if the sky is clear, but if cloudy we need not compute at all, thus saving our time and labor. Furthermore, the computations are very easily made, neither the times of immersion and emersion or of apparent conjunction having to be assumed and the entire data being taken from the *American Ephemeris and Nautical Almanac*, and one table easily prepared beforehand for the particular latitude; it is so simple that a school boy can obtain results equally as accurate as can be obtained by a mathematician by the usual methods, and the entire computation can be gone over and proved in less than *two* minutes.

I originally intended to give but a brief notice of the method for practical observers, but have concluded to describe it at length for the benefit of such amateurs as may be interested in but not familiar with the subject.

The method is partly graphic, the diagram being drawn with reference to a certain scale, from the data above mentioned. This scale is called the "Latitude Scale," the number of inches equal to ten (10) divisions being found by multiplying the cosine of the geocentric latitude of the place of observation by the geocentric radius of the earth at the place, and

dividing unity (1) by the amount so found. This scale is invariable for all computations involving the latitude of the place for which it is constructed, and the quantities O, Y, x', y' and the moon's semi-diameter express the number of divisions of this scale to be taken in making the diagrams.

The occultation is to be projected on a plane passing through the earth's centre, perpendicular to a straight line joining the earth's centre and the star. On this plane we project the earth's central meridian and the parallel of latitude corresponding to that of the observer, (as seen from the star) in the form of an ellipse, the semi-major axis of which is always ten (10) inches long for all latitudes and is perpendicular to the meridian line; the semi-minor axis of which is of a length equal in inches to the sine of the star's declination multiplied by ten, and is coincident with the meridian line. This ellipse represents the apparent diurnal path of the observer, and, the visible pole being placed at the top of the diagram, the semi-ellipse visible from the star will be *above* the major axis when the latitude and star's declination are of an opposite name and *below* it when they are of the same name. The place of the centre of the ellipse on the plane of projection is at a distance from the earth's centre equal to the quantity O, which quantity is found by multiplying together the sine of the geocentric latitude, the geocentric radius of the earth, the cosine of the star's declination and 100. This quantity is to be measured on the meridian line, north from the earth's centre if the latitude is north and south if the latitude is south.

The moon's path on the plane of projection is represented by a straight line, the direction of which is found by means of the co-ordinates x' and y', the former being measured on a line parallel to the major axis of the ellipse and the latter being measured north if prefixed by the sign +, and south if prefixed by the sign — (as indicated in the *Ephemeris*) on a line joining the easterly extremity of x' and perpendicular to it. The place of the moon's centre on the plane of projection at geocentric conjunction in right ascension is at a distance from the earth's centre equal to the quantity Y, which is found by mul-

tiplying the quantity Y, as given in the *Ephemeris*, by 100. This quantity is also to be measured on the meridian line, north from the earth's centre if prefixed by the sign +, and south if prefixed by the sign —, as indicated in the *Ephemeris*. The co-ordinates x' and y' represent the moon's hourly motions in right ascention and declination respectively, and are found by multiplying the quantities x' and y' as given in the *Ephemeris* by 100. The moon's semi-diameter is always equal to 27.23 divisions of the Latitude Scale for all latitudes and occultations.

The ellipse (which for convenience we will designate as the star's path) is to be divided into intervals of five minutes of time by drawing ordinates to abscissæ of lengths equal in inches to the sines of 1¼° multiplied by ten, 2½° multiplied by ten, 3¾° multiplied by ten, etc., up to 90° multiplied by ten, all measured perpendicularly from the minor axis. This dividing of the ellipse is for the purpose of showing at once the place of the star for any moment of hour angle and is the same for all occultations. The moon's path is also to be divided into hours and minutes, the extent of the moon's motion in one hour being exactly equal to the hypothenuse of the right angled triangle formed by the co-ordinates x' and y'.

The moon's and star's paths having been laid out, it will be evident that, given the local hour angle of the star at the time of the moon's geocentric conjunction in right ascension, we know at once the exact places of the star and the moon's centre at that moment; for the place of the former will be at the hour and minute on the ellipse indicated by the hour angle and the place of the latter will, of course, be at the meridian line. Knowing this, and knowing also the extent of the moon's motion in one hour, we can easily find its place for any other time by dividing its path into hours and minutes, reckoning from the meridian line and the time of the hour angle; and the moon's path is therefore to be divided throughout its whole extent for that purpose.

Now as the moon and star move along in their respective paths, there will be two places where the moon's centre and

the star will be separated by an extent equal to the moon's semi-diameter, at the first of which will occur the immersion and at the second the emersion. Now, knowing the local mean, standard or sidereal time at which the moon's centre was at the meridian line (at geocentric conjunction in right ascension) and also knowing as before, the extent of its motion in one hour, we can easily find the time at which its centre was at any other place on its path by again dividing its path into hours and minutes of the same scale as before, reckoning from the meridian line and the time of geocentric conjunction; and its path is again to be divided throughout its whole extent for that purpose. Therefore, the places of the moon's centre relative to this second division of the moon's path will correctly indicate the local mean, standard or sidereal time (whichever it is desired to find) at which the immersion and emersion will take place. Circles may be drawn to represent the moon and the north points and vertices marked. The north point is in a direction from the moon's centre parallel to the meridian line; the vertex is in a direction from the moon's centre parallel to a line joining the earth's centre and the star. The angles which the points of immersion and emersion make with the north point and vertex may now be easily measured.

Now, in order that the diagrams may be drawn with the greatest facility, the computer must supply himself with the following things:

First, the "Latitude Scale." This may be made with pen and ink on the edge of a piece of card, but as the scale is invariable for the place, the divisions had better be cut on wood or metal. The number of inches equal to ten divisions of the scale is to be computed as previously directed.

Second, an ellipsograph. This instrument may be purchased at any mathematical instrument store, or one made for the purpose. The one I use consists essentially of two bars or tracks of ¼ in. square brass placed at a right angle and screwed to a triangular piece of sheet brass. On each of these tracks slides a carrier, which carries a bar at one extremity of

which is fastened an ordinary writing pen. This does the work very quickly and accurately.

Third, a card, on which are drawn the ordinates to the ellipses corresponding to five-minute intervals of time. These ordinates have abscissæ of length equal in inches to the sines of 1¼° multiplied by ten; 2½° multiplied by ten, etc., up to 90° multiplied by ten. Thus, $5^m = 0.218$ in., $10^m = 0.436$ in., $15^m = 0.654$ in., etc., all measured from the minor axis. The diagram paper being laid on this, the ordinates will be seen through it and may thus be easily marked on the ellipse. Only a small portion of the ellipse is required in practice.

Fourth, as the extent of the moon's hourly motion differs at different times, a scale of equal parts for dividing this extent into minutes of time will be required. This "hourly scale" is also to be drawn on card, 52 divisions of the latitude scale across the top and 64 divisions across the bottom—top and bottom divided into six equal parts and one of those parts into ten, and connected by straight lines.

Fifth, the moon's semi-diameter scale, being simply a piece of wood or metal, diamond shaped, the length of which is exactly equal to 27.23 divisions of the latitude scale.

Sixth, a table giving the quantity O and the length of the semi-minor axis for every ten minutes of declination up to 28°.

Seventh, compasses, pen and ink (or pencil if preferred), paper, a T square, a rule divided into tenths of inches, a straight edge about twenty inches long and a protractor for measuring the angles.

To illustrate the working of this method, let us compute the circumstances of the occultation of 37 *Sextantis*, 1886, April 14, for Harvard College Observatory, Cambridge, Mass., the position of which is:

Latitude geographic, ... $= +42° 22'.8$
Longitude from Washington, ... $= -\ 0^h\ 23.7^m$
Difference of time, Standard (75th meridian) and Washington, ... $= -\ 0^h\ 8.2^m$

and the co-ordinates for which are:

$$\log \rho \sin \varphi' = 9.82641$$
$$\log \rho \cos \varphi' = 9.86915$$

Latitude Scale, 10 divisions = 1.3516 inches.

Turning then to the *American Ephemeris*, page 425, we take out and compute the star's local hour angle and standard time of geocentric conjunction thus:

Wash. Hour Angle	$= +1^h\ 7.9^m$
Diff. longitude	$= -0\ 23.7$
*Local Hour Angle	$= +1\ 31.6$
Wash. M. T. of Conj.	$= 10^h\ 15.3^m$
Diff. time	$= 0\ 8.2$
Standard Time of Conj.	$= 10\ 23.5$

Also, we take out from our table for finding O and the semi-minor axis,

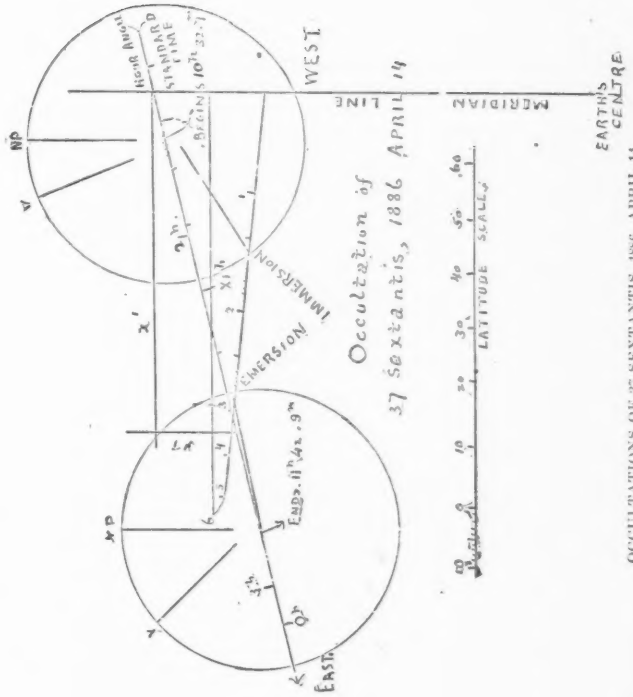
$$O = 66.55 \text{ divisions.}$$
$$m = 1.21 \text{ inches.}$$

and from the *Ephemeris*, mentally multiplied by 100,

$$Y = +76.25 \text{ divisions.}$$
$$x' = 57.90 \text{ "}$$
$$y' = -17.49 \text{ "}$$

and we are now prepared to construct the diagram represented in Fig. 1, at one-fourth the real size. Commence by drawing the meridian line and major axis, so placing them that when the ellipse is drawn the place on it corresponding to the star's hour angle will be at or near the centre of the paper. Now draw the ellipse (or that portion of it required—usually about two hours) and mark the five minute intervals of time on it. Then with the latitude scale measure O south (as the latitude is north) on the meridian line to find the earth's centre and from this point measure Y north (as that quantity is +) to find the place of the moon's centre at geocentric conjunction in right ascension. Next measure and draw the co-ordi-

* Hour angles must always be considered positive, *i. e.*, *past* the meridian.



OCCULTATIONS OF 37 SEXTANTIS, 1886, APRIL 14.

nates x' and y', and draw the moon's path; measure on a slip of paper the extent of the moon's motion in one hour, and, by the aid of the "hourly scale" divide it into ten- and one-minute intervals. Now, placing the star's hour angle at the meridian line, by the aid of this scale divide the moon's path on one side into intervals of ten minutes; then take the moon's semi-diameter scale and find the approximate places of the moon's center at immersion and emersion. The ten minute intervals within which the moon's centre will lie are then to be divided into minutes and the exact places of the moon's center at immersion and emersion found. Then, placing the "standard" time of geocentric conjunction at the meridian line, again divide the moon's path (on the other side) as before, and we have at once the "standard" time at which the immersion and emersion will take place. Circles may now be drawn to represent the moon if desired, the north points and vertices found and the angles measured and the computation is complete, giving the following results, Eastern standard (75th meridian time):

Immersion, at $10h\ 32.7m$ at 124° from the north point and 96° from the vertex.

Emersion, at $11h\ 42.9m$, at 285° from the north point and 245° from the vertex.

Duration of occultation, $1h\ 10.2m$.

At the observatory the immersion was observed by Prof. Searle with the 6¼ in. Clacy refractor at $10h\ 32m\ 17.2s$. The emersion was not seen.

If all occultations visible at a place are to be computed, it will be found very convenient to go over the "Elements" in the *Ephemeris* and select those fulfilling the conditions of visibility, and mark on the margin of the page opposite the particular occultation the quantities O and m. Then, when constructing the diagrams, all the quantities may be taken directly from the *Ephemeris*, and measured at once. I have said that not over fifteen minutes was consumed in computing an occultation by this method, but I very frequently compute one in *ten minutes*, and I think that anyone, having acquired that familiarity with the process which comes with practice, can do the same.

XUM

ON PERSONAL ERRORS IN DOUDLE-STAR OBSERVATIONS.

H. C. WILSON, CINCINNATI OBSERVATORY.

(Continued from page 179.)

FOR THE MESSENGER.

Similarly it may be shown that at 90° (fig. 5, page 175) the parallel measure will presumably coincide with the true angle, while the normal measure will deviate in the directions *nn* and *n' n'* according as the head is inclined to the right or left. It will be seen that the errors of the normal measures increase with the angle from the vertical, while those of the parallel measures increase with the angle from the horizontal; and that both change signs with the quadrants. The corrections to be applied may be determined from the differences between the two classes of measures of the same stars.

The effect of the inclination of the head upon the measures of distance is not so apparent. In my own case, the difference between normal and parallel measures $(n-p)$, is negative near the vertical and positive near the horizontal position, while it passes through zero near 45°. This would seem to indicate that with me the inclination of the head produces a tendency to *exaggerate* distances, while it makes no difference whether the eyes are normal or parallel to the stars.

It may be of interest to give here the results of an investigation of my personal errors during the period from 1882 to 1886. For this investigation I selected those stars which I had observed with both positions of the eyes on the same night, in order to eliminate the effects of bias peculiar to certain nights. Most of the observations were made with a magnifying power of 450. In the preliminary investigation I used only the measures made with that power. Afterward measures made with other powers were included, the differences having been first multiplied by the square root of the ratio of the magnifying power to 450. The difference $n-p$ for each pair of measures of position angle, then gave an equation of the form,

$$e=a+bw+cw^2+\text{etc.},$$

in which w represents the reciprocal of the distance between

the stars, and a, b, and c are coefficients to be determined and which depend upon the angle with the vertical, the method of observing, and the peculiarities of the observer. Dividing the equations into groups depending upon the angle with the vertical and solving by the method of least squares, the following set of equations was obtained :

V	$n-p$	No.
0°	.....	.....
25	..... $e = -0^s.7 \quad -3^s.4\ w$	35
45	..... $= -0\ .4 \quad -4\ .5\ w$	36
65	..... $= -0\ .3 \quad -3\ .3\ w$	26
90	.....	.....
115	..... $= +0\ .3 \quad +4\ .2\ w$	35
135	..... $= +0\ .0 \quad +4\ .7\ w$	33
155	..... $= +1\ .0 \quad +2\ .1\ w$	23

The groups near 0° and 90° were omitted because I had neglected to note the direction in which my head had been turned for each measure. The signs in those groups were therefore discordant and I did not feel at liberty to change them arbitrarily to make them agree. It will be seen that the signs are all negative in the first quadrant and positive in the second, and that the coefficients do not differ more than can be accounted for by accidental errors of observation. Indeed, if we take the means of the three equations in each quadrant we shall find that they are numerically identical. It is probable that the difference $(n-p)$ is constant for any given distance and the corrections n and p will therefore be complementary to each other. I preferred, however, to retain the coefficients as they stand in the above equations and to form from them, by interpolation, a table of corrections, assuming that the error should be equally divided at 45°, and that it should belong wholly to the parallel measures at 0°, and to the normal measures at 90°. After the preliminary corrections had been applied, the arithmetical means of all the observations of each star were taken and from these means the individual, uncorrected observations were subtracted. The resulting differences arranged in groups according to angle and distance are given in the following tables :

	15°		No.	45°		No.	75°		No.
	n	*p*		*n*	*p*		*n*	*p*	
0″—2″	+ 0°.3	−3°.8	8	+ 1°.2	−2°.2	18	+ 4°.3	−0°.7	4
2 —8	+ 0 .2	−1 .6	16	+ 0 .9	−0 .9	20	+ 1 .4	−0 .4	15
8 —32	+ 0 .2	−0 .8	12	+ 0 .5	−0 .5	13	+ 0 .9	−0 .1	6

	105°		No.	135°		No.	165°		No.
	n	*p*		*n*	*p*		*n*	*p*	
0″—2″	−4°.2	+ 0°.8	10	−2°.0	+ 1°.6	15	−0°.4	+ 3°.6	11
2 —8	−1 .4	+ 0 .4	15	−0 .6	+ 0 .3	25	−0 .4	+ 1 .6	16
8 —32	−0 .6	+ 0 .2	5	−0 .3	+ 0 .2	16	−0 .2	+ 0 .7	6

The corrections seem to be practically the same in both quadrants, only in the reverse order; also, the corrections to normal are the same as those to parallel in the reverse order. They may therefore be combined, without regard to sign, in groups which depend upon the angle of inclination of the head, and if the mean distances of the three groups be assumed 1″ 5″ and 16″, we shall have

	15°	45°	75°
1″	± 0°.5	± 1°.7	± 4°.0
5	± 0 .3	± 0 .7	± 1 .5
16	± 0 .2	± 0 .4	± 0 .8

The sign + applies when the head is turned to the left, and sign — when the head is turned to the right. In these groups the corrections are approximately proportional, directly to the angle, and inversely to the square root of the distance. They may therefore be represented with a sufficient degree of accuracy by the general formula

$$c = a\, I \sqrt{\frac{1}{v}}$$

in which I represents the angle of inclination of the head and a is the only coefficient to be determined. Applying this formula to all of the observed differences, I obtained the following values of a for the three groups:

15°	$a=+0.040$
45	$a=+0.038$
75	$a=+0.043$
Mean	$a=+0.040$

The agreement between these values is quite satisfactory and a may be assumed constant.

The above corrections may be perhaps a little more accurately represented by the formula

$$c=a+b\,w+ \text{ etc.},$$

but the coefficients a and b will vary with the angle. The equations for the three groups will be

$$15° \quad c = +\,0°.2 + 0°.4\,w$$
$$45 \quad c = +\,0.3 + 1.5\,w$$
$$75 \quad c = +\,0.6 + 3.4\,w$$

The corrections computed by the two formulæ are nearly identical up to 75°; and observations made with the head inclined more than 75° should be rejected. I prefer therefore to use the first form.

The comparison of the measures of distance gave the following results:

NORMAL—PARALLEL.

	0°	No.	45°	No.	90°	No.	135°	No.
0″— 2″	—0″.07	7	—0″.03	13	+0″.08	2	—0″.01	13
2 — 8	—0 .05	12	0 .00	21	+0 .10	14	—0 .03	26
8 — 32	—0 .15	5	+0 .01	12	+0 .06	2	+0 .03	7
Mean.	—0 .09	24	0 .00	46	+0 .09	18	0 .00	46

These differences do not appear to vary with the distance but do not show considerable change with the angle. The fact that the parallel measures are the greater at 0° and that the normal measures are the greater, by the same amount, at 90°, *i. e.*, the measures in which the head is inclined most are largest, would seem to indicate that the inclination of the head produces a tendency to exaggerate distances, while the fact that the measures are equal at 45° and 135°, where the inclina-

tion is equal in either direction, would seem to show that no effect is produced upon the distances by the different relative positions of the eyes and stars. It is probable that the measures would be improved by the application of a system of corrections something like the following:

°	*n*	*p*
	"	"
0	0.00	−0.09
45	−0.04	−0.04
90	−0.09	0.00
135	−0.04	−0.04
180	0.00	−0.09

The effect of neglecting these corrections would, however, only produce a constant error in the distances of about + 0″.04. I concluded therefore not to apply any corrections to my observations of distance.

In order to test my personal equation by comparison with other observers, I collected all the available measures of the doubles in my working list, rejecting in the comparison all binaries and doubtful measures. The results of the comparison between my corrected measures and those of W. STRUVE, DEMBOWSKI, BURNHAM and the previous Cincinnati observers, are given in the following tables, arranged in groups according to angle and distance:

COMPARISON OF POSITION ANGLES.

	W.—Str.	No.	W.—Dem.	No.	W.—Bu.	No.	W.—C. O.	No.
0°	−0°.1	23	+0°.4	20	+0°.7	5	+0°.6	46
45	+0 .8	10	−0 .9	11	−1 .3	10	+1 .1	44
90	+0 .6	12	−0 .5	13	+1 .0	9	+0 .7	43
135	+0 .7	12	+0 .8	16	+1 .1	9	+1 .3	45
Mean.	+0 .5		0 .0		+0 .4		+0 .9	

	W.—Str.	No.	W.—Dem.	No.	W.—Bu.	No.	W.—C. O.	No.
1″—2″	+0°.6	6	+0°.6	12	+0°.5	8	+1°.8	38
2 —4	−0 .1	17	+0 .1	17	−0 .4	10	+0 .7	42
4 —8	+1 .1	11	−0 .4	12	+0 .3	6	+0 .8	39
8 —16	+0 .1	16	0 .0	14	−0 .1	8	+0 .7	38
16 —32	+0 .3	7	+0 .1	5	.....	..	+0 .8	21
Mean.	+0 .4		+0 .1		+0 .1		+0 .9	

COMPARISON OF DISTANCES.

	W.—Str.	No.	W.—Dem.	No.	W.—Bu.	No.	W.—C. O.	No.
0°	+0″.11	23	+0″.18	20	+0″.07	5	+0″.05	46
45	+0 .19	10	+0 .15	11	+0 .21	10	+0 .07	44
90	+0 .19	12	+0 .25	13	+0 .16	9	+0 .07	43
135	+0 .16	12	+0 .19	16	+0 .19	9	+0 .03	45
Mean.	+0 .16		+0 .19		+0 .16		+0 .05	

	W.—Str.	No.	W.—Dem.	No.	W.—Bu.	No	W.—C. O.	No.
1″—2″	+0″.02	6	+0″.09	12	+0″.22	8	+0″.08	38
2 —4	+0 .09	17	+0 .14	17	+0 .07	10	+0 .03	42
4 —8	+0 .30	11	+0 .33	12	+0 .10	6	+0 .09	39
8 —16	+0 .25	16	+0 .30	14	+0 .39	8	+0 .10	38
16 —32	—0 .05	7	—0 .02	5	.....	..	—0 .05	21
Mean.	+0 .12		+0 .17		(+0 .19)		+0 .05	

Professor STONE found, by comparison with the corrected measures of W. STRUVE, that the position angles in Publication No. 5, require a constant correction of about + 0°.6 (Introduction, page xix). This will explain, in part at least, the nearly constant difference between my measures and those of the previous Cincinnati observers as indicated by the column *w—c. o.* It appears then from the first two tables that my systematic errors in the measurement of position angles have been satisfactorily eliminated.

In the last two tables, on the other hand, all the comparisons conspire to show that my measures of distance, except for large distances, are too great. It is interesting to compare the differences in the last table with the following corrections to the distances of Str., Dem. and C. O., as derived by comparison with O. STRUVE'S results (see Pulkowa Observations Vol. IX, page 131, and Pub. Cin. Obs. No. 5, page xxiii).

	Str.	Dem.	C. O.
1″.6	0″.00	+0″.02	+0″.08
2 .9	+0 .14	+0 .14	+0 .20
5 .6	+0 .22	+0 .22	+0 .22
12 .0	+0 .19	+0 .20	+0 .06
23 .9	+0 .08	+0 .06	—0 .16
	+0 .13	+0 .13	+0 .08

When these corrections are applied my deviations almost entirely disappear. If, then, O. STRUVE'S results are reliable, I may conclude that my measures of distance as well as of angle are nearly free from systematic error.

Mt. Lookout, April 15, 1886.

EDITORIAL NOTES.

The next number of the MESSENGER will be published for September, as August will be one of our vacation months.

The meeting of the American Association for the Advancement of Science for 1886 will be held in Buffalo, N. Y., beginning August 18. Accommodations at the large high school building of that city have been secured, and adequate local arrangements are assured, and a large meeting is anticipated. For particular information address the local Secretary, Dr. Julius POHLMAN, Buffalo, N. Y.

DISCOVERY OF COMET BROOKS NO. 3, 1886.—On Saturday evening, May 22, 1886, I discovered a new comet in the head of *Virgo*, its approximate position being R. A. $11^h\ 45^m$ and Decl. $+10°$. Although in a nebulous region, I at once suspected its cometary character, and prompt telegraphic notification was made to Dr. SWIFT of the Warner Observatory. The next evening I was enabled to confirm the discovery by a second observation, as also did Dr. SWIFT the same evening, both observations being made with difficulty through thick haze.

The comet is large, nearly round, and not a difficult object in a fair sized telescope. An observation obtained last evening gave its estimated position, R. A. $11^h\ 58^m\ 30^s$ and Decl. $+6°\ 01'$.

WILLIAM R. BROOKS.

PHELPS, N. Y., May 29, 1886.

From the Albany positions of May 25, 28, and 31, Mr. H. V.

EGBERT has computed the following elements, which are published by permission of the Director of the Dudley Observatory.

ELEMENTS.

$T = 1886$, May 28.76, Greenwich M. T.

$$\left.\begin{array}{rl} \pi - \Omega = & 170\ 13 \\ \Omega = & 46\ 03 \\ i = & 16\ 59 \end{array}\right\} \text{Mean Eq. } 1886.0.$$

$q = 1.5122$

Light May 25 = 1.0.

The comet is about one and one-half minutes of arc in diameter and faint, with an ill defined nucleus in the n p part.—*Science Circular, No. 70.*

The elements of this comet have also been computed by Dr. H. OPPENHEIM, varying but little from those given by Mr. EGBERT. We are not aware that an ephemeris of this comet has been computed by any one beyond June.

OBSERVATION OF COMET *c* (BROOKS) 1886.—Ring micrometer observation of Comet *c*, made at Vanderbilt University Observatory, as follows:

Nashville mean time, 1886, May 24, $8^h\ 46^m\ 48^s$.
Apparent right ascension, $11^h\ 52^m\ 43.82^s$.
Apparent declination, $+8°\ 20'\ 26.2''$.
From four comparisons with Yarnall 5021,
Comet preceded star by $2^m\ 28.52^s$.
Comet south of star $12'\ 27\ 7''$.
Mean place of star for 1886, .0.
Right ascension $11^h\ 55^m\ 11.05^s$.
Reduction to apparent R. A. $+1.29^s$.
Declination, $8°\ 33'\ 1.0''$.
Reduction to App. Decl. -6.5^s.

E. E. BARNARD.

THE DOUBLE STARS 30 AND 41 GEMINORUM.—The wide

unequal pair 30 *Geminorum*, the discovery of which is attributed to Mr. EDGECOMB in 1878, appears to have been first noted by Lamont in 1836. The only measures I know of are:

Lamont, 185.1° : 32.00″ : 1836.24
β, 184.1 : 28.05 : 1879.06

The difference is probably due to the proper motion of the large star. The neighboring star, ξ (31) Geminorum, has also a distant *comes* according to the Munich observer. ξ has an annual proper motion of —0.0087 seconds and —0.195″ (Auwers). In his note on 41 Geminorum (θ Σ 162) in the 47th volume of the "Memoirs" of the R. A. S., Mr. Burnham remarks that the measures of Maedler and his own "would seem to indicate considerable change if the early distance is correct," (of also *Memoirs* R. A. S., vol. xliv) and he gives the following results:

Maedler, 164.9° : 13.5″ : 1843.22
β, 154.9 : 21.16 : 1879.15

This eminent observer has, however, apparently overlooked Maedler's note on this object appended to his single observation, recorded in the 11th volume of the Dorpat Observations, page 53, "distanz geschatz." Taking all the circumstances into consideration, it would perhaps be premature to suggest any real change in the angle. DEMBOWSKI only appears to have looked for this object once, and notes "1865.97, veduta nessuna compagna." It was rejected from the revised catalogue of O. STRUVE because the distance was over 16″, and the magnitudes of the companion less than 8—9.

HERBERT SADLER.

LARGE PROPER MOTION IN A SMALL STAR NEAR 70 OPHIUCHI.—In the "English Mechanic" for July 14th, 1882, I drew attention to the very large proper motion of a star near 70 Ophiuchi (Σ 2272), and again in the pages of the same periodical for July 10th, 1885. I believe that the proper motion in question is by far the largest hitherto known to occur in the case of so small a star, for Professor HALL rates it about 13

mag., though as Se. called it 11 mag. in Σ's scale, the Washington observer may have estimated it rather too faintly. It seems to have been first noticed by the late Admiral SMYTH, as a little star about 5.5 seconds of time south preceding 70; but the only measures I know of are as follows:

Secchi, 215.08°: 87.514″. 1856.627
Hall, 197.84 : 71.384. 1878.842

The annual proper motion of 70 Ophiuchi is about 1.13″ in the direction of 169.5°, and that of this small star 1.28″ in about 82°, or larger than that of 70 Ophiuchi itself, and far larger than any hitherto known to belong to so faint a single star (for such the distance of this faint *comes* from the bright pair entitles it to be called); as FLAMMARION'S ascription of a large proper motion to a small star between 36 Ophiuchi and 30 Scorpii is obviously untenable, and indeed rests wholly on the presumed accuracy of a diagram by Admiral SMYTH. The *measures* of that observer are not usually considered to be very reliable now-a-days; his diagrams resemble nothing in the heavens above, or in the earth beneath. In view of the very large deviations of the motion of the components of 70 Ophiuchi from the orbits constructed from the best observatories, deviations which are increasing with very great rapidity, and to which I drew attention in my note in the above mentioned paper of July 10th, 1885, it is desirable that this small star should not be neglected by those astronomers who measure the bright pair, though it may exceed the ordinary limits of distance. There is another small star in 49.59° : 87.209″ : 1878.84 (HALL) which was only observed in position angle by SECCHI (67.2°). The change is probably due to the proper motion of 70 Ophiuchi itself. HERBERT SADLER.

London, June 9th, 1886.

ORBIT OF COMET *b* 1886 (BROOKS).—From Prof. FRISBY'S observation of May 3d, and my own of May 1st and 6th, comprising an arc of some 10° in geocentric longitude and 8° in latitude, I have computed the following orbit of Comet *b* 1886

(BROOKS). The observations have been corrected for parallax and aberration, and the orbit carried to a second approximation, a third being unnecessary.

$$T = 1886, \text{ May } 5.02796, \text{ Gr. M. T.}$$

$$\left.\begin{aligned} \pi - \Omega &= 39\ 46\ 12 \\ \Omega &= 288\ 34\ 1 \\ i &= 101\ 1\ 39 \end{aligned}\right\} \text{Mean Eq. 1886.0.}$$

$$\log q = 9.92562.$$

Middle Place $(c-o)$

$$\Delta\lambda \cos\beta = -5''$$

$$\Delta\beta = +2$$

O. C. WENDELL.

Harvard College Observatory, June 10, 1886.

THE GREAT LICK EQATORIAL.—The contract for mounting the 36-inch objective has been awarded by the Lick Trustees to Warner & Swasey, of Cleveland, for $42,000. The telescope is to be fifty-seven feet long; the diameter of the tube is forty-two inches. The tube is suspended at the middle, and the point of suspension is to be thirty-seven feet above the floor of the dome. The axes on which the tube moves are supported by a heavy iron column 17x10 feet at its base. Provisions are made by which it is possible for the observer at the eye end of the telescope to command all the possible motions, and these same motions can also be controlled by an observer stationed on a small balcony twenty feet above the floor. It is expected that in spite of the great size of the telescope itself and of its great weight, the mechanism will be so delicately adjusted as to require little physical force. Messrs. Warner & Swasey are to have this mounting completed in April, 1887, and sometime during the summer of 1887 the glass will be brought to Mount Hamilton and put in place. The cost of the entire apparatus is as follows: Cost of the dome, $56,850; cost of the mounting, $42,000; cost of the visual objective, $53,000; additional cost of the photographic objective, $13,000. Total, $164,850. Besides these sums several thousand dollars

will be required to put the instrument into its final completed state.

NOTE ON REPOLISHING SURFACES OF ROCK-SALT.—In the Proceedings of the American Association for August 1885, Mr. J. A. BRASHEAR describes a method for producing optical surfaces of rock-salt which in his hands gives results leaving little to be desired in the way of accuracy of figure and brilliancy of polish. These surfaces, however, when exposed to the air, deteriorate more or less rapidly, according to the hygrometric conditions at the time, and soon become opaque and unfit for use. As the change in an ordinary dry state of the atmosphere is a gradual one, it is often a rather nice question to decide when it has advanced to such a point as to render the surfaces unfit for their work. The production of brilliant surfaces by Mr. BRASHEAR'S method requires, besides the necessary appliances, much experience in such manipulations, doubtless well worth acquiring, but not always in the possession of the experimenter, and thus the restoration of the original polish when dimmed will in general be beyond his power.

It very frequently happens, however, in investigations in radiant heat, where rock-salt finds its chief use in physical research, that the extreme accuracy of figure obtained by the above process is by no means necessary, whereas great transparency, *i. e.*, with good material, brilliancy of polish of the surfaces is always absolutely essential. Under these conditions it is convenient to have some rough and ready way of renewing the polish of the dimmed surface, even at the expense of its perfection of figure.

After considerable experimenting with different substances, I have found nothing which gives better results for this purpose than thick, soft Canton flannel. It should be spread out on a smooth flat surface like a table top, or, better, a marble slab, with the furry side up. Breathe evenly over the surface of the rock-salt prism or plate and rub it quickly with circular and then with straight strokes upon the flannel. As soon as it glides easily, without much friction, remove it and examine

the surface, and if not bright repeat the operation, using a different part of the cloth. It is best to wear a pair of kid gloves, to avoid the condensation of moisture from the hands. A surface almost as bright as the original can be produced in this way, and the irregularities of figure caused by the rubbing are surprisingly small. A prism by Mr. BRASHEAR, which had been treated as many as eight times in this manner by the writer, still defined the Frauenhofer lines with considerable sharpness, and the refracting angle had been altered less than 1′. The surfaces, when examined with a test plane by means of interference bands in sodium light, showed curious irregularities and a general slight convexity of figure.

Lenses may be treated in the same manner, the cloth being held in the hand, unless a rounded surface of approximately the curvature of the lens is at command, but as they are usually thin they must be handled with great caution. It will of course be understood that nothing can be done in this way with surfaces originally bad, nor can much improvement be made in surfaces which have been badly corroded by exposure in too moist an atmosphere. The process applies only to surfaces of good figure which have become dimmed by exposure under ordinary conditions. After being repolished in this way as many times as experience shows is allowable, the surfaces must be treated by a more perfect process like Mr. BRASHEAR'S in order to restore their original accuracy of figure.

J. E. KEELER.

DISCOVERY DATES OF BROOKS' COMETS *a*, *b*, *c* 1886.—The correct dates of discovery of the Brooks Comets *a*, *b*, *c* 1886, are as follows:

Comet *a*, on the evening of April 27.

Comet *b*, on the morning of May 1.

Comet *c*, on the evening of May 22, civil reckoning.

On page 186 of the June MESSENGER the discovery of Comet *a* was given wrongly as April 28.

A New Minor Planet, No. 258 magnitude 11, was discovered by Dr. LUTHER, at Dusseldorf, May 7.

OCCULTATIONS OF STARS IN THE HYADES, OBSERVED AT THE DAVIDSON OBSERVATORY,

SAN FRANCISCO, CAL., FEB'Y 12TH, 1886.

$\varphi = +37° 47' 24.1''$

$\lambda = 8^h 09^m 42.52^s$ W. of Gr.

No.	Star.	Dis. or Reap.	Observer.	Instruments, Etc.	Time Noted by Chronom.	Corr'n to Chron.	TRUE LOCAL TIME. Mean.	TRUE LOCAL TIME. Sidereal.	Remarks, Etc.
							February 12, 1886.		
				in.	*h. m. s.*	*m. s.*	*h. m. s.*	*h. m. s.*	
1	Theta (2nd) Tauri	Dis ..	D.....	6.4 Equat., 90 diams. Sid.* 3479 ..	2 29 49.2	+0 01.50	4 57 51.34	2 29 50.70	Good. Daylight.
2	75 Tauri	" ..	".....	do. do. do.....	2 35 37.2	01.50	5 03 38.39	2 35 38.70	Faint and difficult.
3	Theta (2nd) Tauri	Reap.	".....	do. micr. eyepiece 240 diams.....	2 58 38.+	01.52	.....	.....	See note (*a*) below.
4	Theta (1st) Tauri.	" ..	".....	do. do. do.....	3 17 52.3	01.54	5 45 46.61	3 17 53.84	Sharp. C. B. H. marking time, etc.
5	B. A. C. 1394.....	Dis ..	".....	do. eyepiece 40 diams.....	3 33 15.1	01.55	6 01 06.90	3 33 16.65	Good. do. do.
6	B. A. C. 1391.....	Reap.	".....	do. micr. eyepiece 240 diams.....	4 47 55.1	01.63	7 15 34.74	4 47 56.73	Possibly ½ sec. late. do.
7	D. M. +16°: 619...	Dis ,	".....	do. power 150 diams.....	5 22 05.4	01.66	7 49 39.47	5 22 07.06	Very faint, but good. D. marks time.
8	B. A. C. 1406.....	" ..	".....	do. do. do.....	5 24 04.9	01.66	7 51 38.65	5 24 06.56	Very good.
9	D. M. +16°: 618...	" ..	".....	do. do. do.....	5 27 01.5	01.67	7 54 34.77	5 27 03.17	Doubtful—faint.
10	B. A. C. 1406.....	Reap.	".....	do. micr. eyepiece 240 diams.....	6 41 48.5	01.76	9 12 09.12	6 44 50.26	Faint;—late one second ?
11	*a* Tauri	Dis ..	".....	do. do. do.....	6 50 48.2	+0 01.77	9 18 07.85	6 50 49.97	Good—Instantaneous. See note (*b*).
12	*a* Tauri	" ..	C. B. H.	3*in.* Recon., 105 diams., M. T.* 231	9 29 39.8	−11 22.73	9 18 08.07	6 50 50.19	Good. "Dropped out of sight."
13	*a* Tauri	Reap.	G. D...	As before.....	8 03 21.2	+0 01.84	10 30 29.03	8 03 23.04	Good—fine ruby red appearance.
14	*a* Tauri	" ..	C. B. H.	As before.....	10 41 51.7	−11 22.72	10 30 28.98	8 03 22.98	Flashed out sharply at this instant.

NOTES.

No. 3 (*a*).—Actual reappearance missed; first seen at this time and estimated 8 or 10 seconds late.

No. 11 (*b*).—Did not have chronometer beat, but counted from inst. of disappearance. Original record of 48.3 seconds—perhaps better.—D.

A working list was prepared as follows: the apparent place of the moon was computed for each hour of G. M. T., and plotted on section paper, as were also all the stars in that locality. The path of the moon being established, the times and angles of disappearance and reappearance were read off approximately, etc. The average error of the prediction was −0.5*m.*, maximum 4.2*min.*

OBSERVERS.—D.=Prof. George Davidson.

C. B. H.=Chas. B. Hill.

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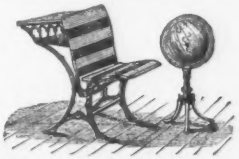

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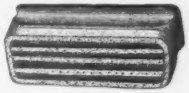

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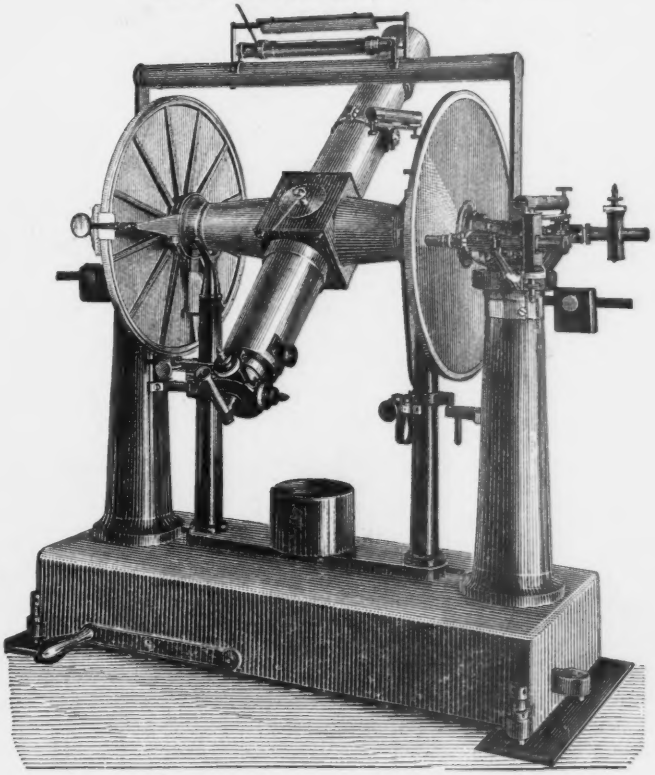

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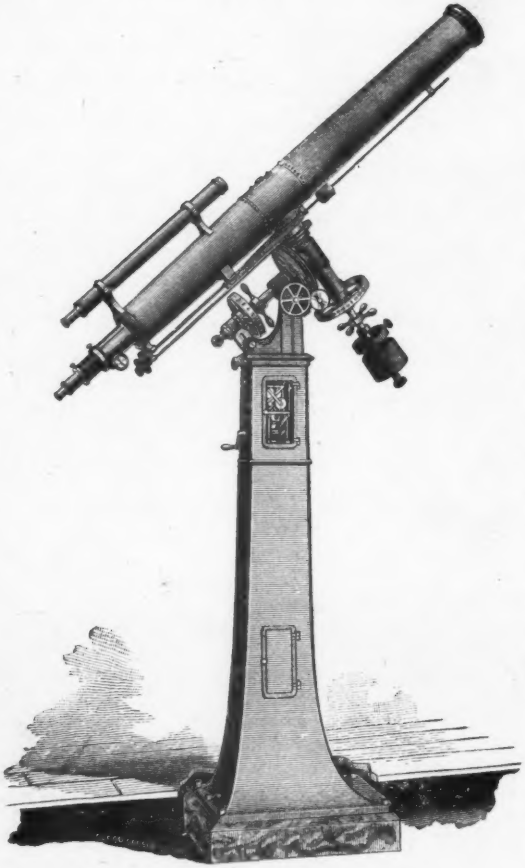

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